LETTERS TO THE EDITOR.

***Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The failure of foreign trees on American soil.

ALLOW me to enter a respectful protest against the sweeping judgment of Professor Sargent in condemnation of foreign trees, which you publish approvingly in your issue of March 4. Though there is, no doubt, a great difference between the climate of this continent and that of Europe, and though unquestionably tree-growth is most dependent upon climatic conditions, yet it would be unwarrantable, from its failure in one place or even several places in this country in ornamental plantations, to generalize upon the adaptability of an exotic species for *forestry* use. It seems to be generally overlooked, if not unknown, in this country, that forestry and arboriculture, or tree-planting as practised by the horticulturist or landscape-gardener, are not the same thing, but in their objects, and consequently in their methods and results, are entirely different. While in ornamental planting the individual tree is the object, and its form in its unity and the development of its beauty is the aim of the planter, forestry has to do with an aggregate of trees, which, properly placed and grouped together, grow and develop very differently from the single tree, or even group of trees, on the lawn. The European larch, even in its native country, does not make a desirable lawn-tree in every locality, and, coming originally from the highest mountain elevations, even as a forest-tree, it requires, when grown upon the plain, particular conditions and special management to secure a thrifty growth, and the quality and quantity of timber for which the tree is noted. I have often pitied those in this country who have expected these results without paying attention to the requirements of the tree. As to the Norway spruce, of which Professor Sargent speaks so disparagingly, I have not seen a finer ornamental conifer of its kind on this side of the Atlantic; and though, as is the case with all the conifers, a time arrives when it loses its peculiar beauty, I doubt whether it does so sooner than any others, while, as a forest-tree, it needs only proper conditions and management, I venture to say, in order to attain the size and quality which it shows in its native country. Plant the Norway spruce in dense groves, on a northern or north-western exposure, with the European larch sparingly interspersed, and no planter will live long enough to see these two, thus united, fail in their onward development.

The Scotch-pine, on poor but deep sands on the western prairies, I am sure will make useful timber sooner than the white-pine. The white-pine was introduced into Germany on large areas about ninety years ago. Growing with great rapidity, and yielding astonishing quantities of wood per acre, the quality of the wood was found to be very inferior until recent years. Experiments have lately shown that the white-pine requires ninety years to make wood of as good quality as the Scotch-pine will produce in seventy years under similar conditions, just as different grains will require different lengths of season in which to mature. These experiments and the many similar ones which could be cited should teach us to be chary of generalizations upon our scanty experiences in forestry in this country.

Of the European willows, so far as osier-growing is concerned, only one, Salix purpurea, seems to have been found adapted to our climate, while several native ones promise success if properly treated.

While I am a most earnest advocate of seeking for the best in that which we have ourselves, and while I advise the planting first of our native trees, with a special study of their requirements, I must deprecate any know-nothing movement against the good things which we may import. Especially let us remember that New England constitutes, territorially and climatically, but a very small part of our country, and that conclusions drawn from experiments there may not be applicable to other portions of it.

B. E. FERNOW.

Washington, March 7.

Inertia-force.

I had thought that my pamphlet, 'Elementary ideas,' etc., might awaken discussion, and possibly bring about a better understanding among teachers of physics as to the interpretation of certain familiar terms. The discussion has evidently begun. Let us not despair of the better understanding.

Having made, however, one direct attempt to explain to Professor MacGregor my use of the term 'inertia-force,' with the sorry result of disgusting him by the use of "language which is not the current language of dynamics," I shall for the moment adopt a different course, and find a little fault with his way of stating things.

Professor MacGregor accepts fully the doctrine stated by Maxwell in a passage quoted in my first letter, that "all force is of the nature of stress, that stress exists only between two portions of matter," and that "the stress is measured numerically by the force exerted on either of the two portions of matter." I will undertake to show wherein his reasoning seems to me to be inconsistent with this doctrine. He takes my illustration of a railway-train which is being set in motion by a locomotive, and says, "If F is the pull of the locomotive, R the frictional resistance, M the mass of the train, and a its acceleration, we have undoubtedly, by Newton's second law of motion,

$$a = (F - R) \div M.$$"

To this every one will agree. Now, with Professor MacGregor's permission, I will put this equation in the form

$$F = R + aM.$$

F is, by his own statement, a force, — the force exerted *by* the locomotive *on* the train. By the doctrine stated by Maxwell, which Professor MacGregor accepts, the force exerted *by* the train *on* the locomotive is also equal to F. It is therefore equal to, and may be expressed by, the terms $R + aM$. Now, one part of this force, the part R, is accounted for by the resistance of friction transmitted through the train to the coupling of the locomotive. How shall we account for the other part of the whole force exerted by the train on the locomotive, the part aM? I call it the *inertia-force*, — the force, or resistance, which the train, *by virtue of its inertia*, exerts on the locomotive which is setting it in motion. I think I can be persuaded to drop the term 'inertia-force,' if a more accurately descriptive one can be adopted; but Professor MacGregor, if I understand him, does not object to the term merely. He denies that the train offers any resistance by virtue of its inertia. But in

denying this he seems to me to reduce the force exerted *by* the train *on* the locomotive to the quantity R alone; and since R is less than F, the pull exerted *by* the locomotive *on* the train, he thus abandons the doctrine that "all force is of the nature of stress," and that "the stress is measured numerically by the force exerted on either of the two portions of matter."

The quotation which Professor MacGregor makes from Poisson I shall not attempt to discuss at length; for I am not familiar with his writings, and do not know exactly what meaning he attached to the word *résistance*. If he used this word as I understand Professor MacGregor to use it, to indicate an *opposing force*, and if he was at the same time committed, as I understand Professor MacGregor to be, to the view that one force always implies an equal and opposite force, then I can only say that I think Poisson was wrong in one part or the other of his doctrine.

E. H. HALL.

Cambridge, March 5.

Comparative taxation.

While I cordially accept all Mr. Henry B. Gardner's statements in regard to the insufficiency of my study of the comparative taxation in Europe and America, I cannot accept his conclusions. He says, in fact, "The inadequate scope of the work has in large measure destroyed the value of the study." To this I cannot agree; and my witness is Mr. Gardner himself. My work has brought out his intelligent criticism, and has turned the attention of himself and of very many other persons to the importance of developing the science of comparative statistics, which is what I have aimed at.

It is very true that I have not attempted to compare the relative taxation of cities, towns, and other subdivisions of states in Europe with those of America; it is very true that some of the cities of this country are excessively taxed as compared to those of Europe: all the more reason for a complete study of the subject. Where are the materials for such an investigation? I have given, to the best of my ability, the relative burden of *national* taxation. I stated that this part of the taxation of countries should be considered separately from that of the towns and cities, for the reason that in Europe a very large part of the national taxation is expended for *destructive* purposes or for the support of privileged classes; while, with the exception of a few cities in this country, the revenues derived from local taxation are paid out for *constructive* purposes both there and here; and on the whole, in spite of the cumbersome nature of the collective work of cities, counties, and towns, the people of this country get about seventy-five cents' worth on a dollar for what they pay in municipal taxes.

Moreover, although Mr. Gardner may not be able to find exact returns of taxation in European countries corresponding to the *per capita* figures which I have submitted, yet I claim to have proved them after as complete examination as is open to a private and unofficial person who does not read German. I hold that the revenue of state forests, mines, and other instrumentalities of subsistence which are often controlled in Europe by governments, constitute as true a tax upon the people as if they had been assessed directly upon their property; and I am of opinion that I have understated the burden of national taxation in Europe rather than overstated it. Suffice it that the figures have attracted attention; and it may be that within one, two, or three years a complete comparison of national as well as state, county, and town taxation may become possible. I should be glad to see Mr. Gardner try his hand, not so much in criticising my work, as in preparing more accurate and more complete tables.

EDWARD ATKINSON.

Boston, March 5.

On the flight of birds.

The wing is extended upward from the horizontal position by the deltoid and the latissimus dorsi muscles to a line which is perpendicular to the body, and is quickly again depressed to the horizontal position by the pectorales. This constitutes the first stage of the 'stroke.' 'Recover' is initiated by an inward rotation of the humerus, semiflexion of the wing at the elbow (the pinion remaining extended and directed obliquely downward and outward), and is carried well forward to a degree sufficient, when seen in profile, to conceal the head. In this position the primaries are semirotated so as to present the least amount of surface to the air in the direction in which the bird is moving. The impetus excited by the stroke carries the bird upward and forward. In the second stage of 'recover,' the humerus is rotated outward, the arm is quickly raised, the primaries restored to the position seen in the bird at rest, and the wing is a second time in the position for the 'stroke.' In the eagle and the hawk the legs are in the position of the 'stroke' when the wings are similarly placed. During the 'stroke' the legs move backward. This motion continues during the 'recover' of the wing, so that the time of the 'recover' of the wing is also that of the 'recover' of the leg. The action of both wings and feet, since both pairs act together, is what I propose to call 'synadelphic.'

The study of the flight was confined to the eagle, the hawk, the pigeon, and the parrot, in the series of instantaneous photographs taken by Mr. Edward Muybridge, under the auspices of the University of Pennsylvania.

HARRISON ALLEN.

Philadelphia, March 7.

On the serpentine of Syracuse, N.Y.

An especial interest attaches to this rock for two reasons: 1°, because of the almost total absence of rocks of this class, or indeed of any intrusive rocks, from the undisturbed paleozoic strata of New York; and, 2°, because of the importance which has been recently attributed to it by Dr. T. Sterry Hunt, as affording evidence in favor of his chemical precipitation theory of the origin of serpentine.

The Syracuse serpentine was discovered in 1837, and was described by Vanuxem in his third annual report in 1839 (pp. 260 and 283), and in his final report on the geology of the third district in 1842 (p. 109). It is also mentioned by Beck, in his 'Mineralogy of New York,' as a 'dike or bed' (1842, p. 275). Dr. Hunt published an analysis of this rock in the *American journal of science* for 1858 (xxvi. p. 236), and has laid great stress upon it in his recent essay on the geological history of serpentines.

Through the courtesy of Prof. A. H. Chester of Hamilton college, the writer has been enabled to study a very complete suite of this rock and its associates, which was collected by the late Prof. Oren